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A FIELD NATURALIST'S SCIENCE.

The Primrose and Darwinism. By a Field Naturalist, M.A. Camb. Pp. xiii + 233. (London: Grant Richards, 1902.) Price 6*s.* net.

IT is hard to tell why this book was written. The preface alone is enough to condemn it, for in the preface we have in miniature the chief defects of the book—inaccuracy and want of scientific method.

At the foot of p. vi. the author makes the astonishing statement that Darwin's predecessors are to be

> "commended for strictly subordinating theory to natural facts. They thus happily avoided the error into which Darwin, in this instance at least, most assuredly and most conspicuously fell."

On p. vii. the author continues,

> "We consider that it was most unfortunate for Natural Science that Darwin relied almost so exclusively on artificial observation, or, in other terms, on experiment, for the investigation and interpretation of natural laws in facts connected with the fertilisation of flowers."

That is to say, the botanists are to be commended for not having attempted to solve the problem of the sexual relation between the two forms of dimorphic flowers in the only possible way in which that question can be attacked.

The preface is followed by a chapter of three pages in which some technical terms are defined, and an incomplete account of dimorphism is given. This, we gather, is intended for the general reader, but it is useless for the purpose; indeed, we fail to see what interest such a reader can find in the book. If he is convinced by the Field Naturalist, and consequently gives up his belief in Darwin's work on the fertilisation of flowers, what does he gain? He has lost a coherent and interesting doctrine which, whatever may be its faults, is undoubtedly in agreement with an enormous range of authentic observation and experiment, and one that has stood the test of time, having been before the world in its modern shape since 1862. The reader is told by the Field Naturalist that *Arum maculatum* is a "purely self-fertilised flower"; if his faith endures up to this part of the book, and if he accordingly swallows the statement, he must for the future give up all attempt to find a function for floral structures, the whole build and habit of reproductive mechanism having become meaningless. It is the same throughout the book; we have pages of weak argument directed against well-authenticated conclusions—arguments which, if accepted, would leave floral structures unexplained and inexplicable. And when the author ventures on suggesting a function, we are liable to come across such a theory as that the orifice in the carina of Lotus (through which pollen is obviously and visibly pumped out) is to serve for the ventilation of the pollen stored within the carina!

If the general reader desires to read this book, let him prepare himself by reading a discussion on floral biology by some candid and competent person. And if, after he has seen how the subject can be treated by a rational writer, he still insists on reading "The Primrose and Darwinism," we must leave him to his fate, though we shall continue to be sorry for him.

We do not propose to go through the whole book, but to discuss one or two points and to leave our readers to judge of the remainder; we must indeed confess that we have found it impossible to read the whole.

The author's principal objection to Darwin's experiments is that in order to exclude insects he made use of a covering of netting. This treatment the author assumes, without any evidence, to be injurious to the pollen. He gives (p. 8) his general reasons for believing in this astonishing conclusion.

> (1) "The influence of the solar rays would be greatly diminished," and "would be much debarred from exercising their full maturing power on the anthers, . . ."

This may or may not be the case; but what botanist would make such a statement, unsupported by a single experiment?

> (2) "Radiation would likewise be almost entirely prevented by the net, and the dew would consequently fail to fall on the anthers. . . . In the mornings of early spring, ... we have frequently found the flowers of the primrose bedrenched with dew."

The general reader would naturally assume that pollen is known to be improved in quality by being wetted. No botanist would have been likely to frame such a hypothesis, for it is well known that pollen is injured by water. And when a Field Naturalist theorises in this way, without giving a shred of experimental evidence, he must be plainly told that he gives up all claims to be considered a man of science.

> (3) "In calm weather" the net would prevent "the free access of the wind, and would prevent it from shaking, and so from freely disturbing and distributing the pollen."

Here again not a particle of evidence is given for his point of view.

In Chapter iv. the author gives instances intended to prove the injurious effect of the net. They are quoted from Darwin's "Cross and Self-Fertilisation." One is the case of *Salvia tenori*, which when protected by a net was quite sterile except for two or three flowers on the summit of the spikes which touched the net when the wind blew.

The Field Naturalist (p. 11) says:—

> "To attribute the capacity for fertilisation" in the unprotected "flowers to the bees is perfectly gratuitous, as the flowers under the net (when bees were excluded), 'when they touched the net and the wind blew,'[1] produced seeds without any cross-fertilisation."

How the Field Naturalist accounts for the flowers which touched the net when the wind blew being in a more natural condition than those which did not touch it we are unable to guess. He does not show that they were more wetted with dew, and gives, in fact, nothing that can be called a reason for his conclusion. Yet he has to account for a striking difference, since those flowers that did not touch the net were quite sterile. How Darwin accounts for the fertility of the flowers is not clear. They may have been visited by bees. Why does not the Field Naturalist go into this point, which would probably entail a discussion of the several types of floral structure found in the genus Salvia? We also look for a discussion of the question whether or no the fact that the flowers were pressed against the netting could so far disturb the mutual relations of the parts as to bring about self-fertilisation.

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[1] An incorrect quotation.

The same want of full discussion is felt in his critique on Darwin's observation on the broom (*Sarothamnus scoparius*). Darwin says the flowers were

> "extremely sterile when the flowers are neither visited by bees, nor disturbed by being beaten by the wind against the surrounding net."

The Field Naturalist quotes this passage incorrectly, omitting "when the flowers are neither visited by bees"—and these words are of some importance, for they show that the disturbance by the wind was believed to have an effect analogous to that produced by insects. In another passage ("Cross and Self-Fertilisation," edit. ii. p. 164) Darwin states that "if the flowers are not dashed by the wind against any object, the keel never opens." If the flower is not opened the style remains in the keel surrounded, but either not fertilised or imperfectly fertilised by its own pollen. But if the style and stamens are freely exposed, as would be the case in the flowers opened by the wind's action, the flowers may conceivably be "dashed against" each other and fertilised, even if they cannot be visited by insects. The whole economy of the broom flower should be thoroughly known, and, indeed, fresh observations should be made, before it is possible to draw the conclusion of the Field Naturalist, that the sterilising action of the net accounts for the results.

The same sort of thing is put forward in the case of *Reseda lutea*. Here Darwin says,

> "The bees were able to suck the flowers through the meshes, and brought pollen to them from the neighbouring plants."

The Field Naturalist says (p. 13),

> "The bees could not possibly reach with their proboscis the side or inside flowers, yet 'the branches were loaded with capsules.'"

As the Field Naturalist was not present when the bees brought pollen to the mignonettes, and as one of the best observers in the world was present, we need not waste more time over this case.

In "Cross and Self-Fertilisation," from which the Field Naturalist takes his cases, he might have found, if he had looked for them, facts which he will find difficult to fit to his theory of the "denaturalising" effect of the net. Darwin gives ("Cross and Self-Fertilisation," edit. ii. p. 357-369) two lists:—(1) Containing plants which, when insects are excluded, are quite sterile, or produce, as far as he could judge, less than half the normal number of seeds. (2) Containing those which, when protected from insects, are either quite fertile or yield more than half the normal number of seeds. The Field Naturalist is bound to account for the fact that many plants are extremely fertile under the net, and he must account, too, for the fact that, broadly speaking, there is a difference between the type of flower found in the two classes. Or, to put it more accurately, in the first or sterile lot 65 per cent. of the genera have asymmetrical or otherwise specialised flowers, while in the fertile lot the specialised genera are 43 per cent.

Why, according to the Field Naturalist's view, should the net be less hurtful to the simple unspecialised flowers? According to the rational view of the matter taken by most botanists, it was to be expected that specialised flowers would be more highly sterile, when insects are excluded, than simple unspecialised flowers. But it is useless to argue thus, for if a female diœcious plant were placed under a net, and were found to be sterile, the Field Naturalist would doubtless account for its sterility by the denaturalising influence of the net, not by the fact that pollen could not reach its stigmas.

Chapter xiii., on "The So-called Dichogamous Plants," may be taken as another instance of the Field Naturalist's method of treating a scientific problem. His notions on this subject seem to be taken from Lord Avebury and Dr. Wallace, neither of whom can rank as original authorities on the question, while we look in vain for references to Sprengel, Delpino and Hildebrand. This is only one instance of the author's ignorance of the literature of his subject. It is probably in consequence of want of knowledge that he sets up an incorrect definition of dichogamy against which to direct his arguments. He ought to be aware that dichogamy does not necessarily mean a complete separation in time of the staminal and stigmatic functions. However, allowing this serious flaw in his point of view to pass, let us see how he deals with a strongly dichogamous species, *Arum maculatum*. He tells us that no flower gives clearer evidence of its own self-fertilisation than the Arum. Yet he quotes an observation of Darwin, who saw minute flies emerge from an Arum, dusted with pollen, and subsequently visit a neighbouring plant. Here Darwin found pollen within the spathe, though the stamens had not burst. No one will pretend that this one observation is conclusive, but it points clearly to the view accepted by botanists that flies carry pollen from the older to the younger spathes, thus fertilising the female flowers[1] before the pollen in the spathe is ripe.

The Field Naturalist principally devotes himself to trying to prove that the Arum does not imprison flies with sufficient regularity and in sufficient numbers to be of any use. He writes (p. 80):—

> "About the forced imprisonment of small flies, Darwin says, 'this statement has [now] been shown by Hildebrand to be erroneous,' and Darwin proves that it is incorrect, and that the small flies can escape before the hairs above wither, by his own experiment" ("Cross and Self-Fertilisation," edit. ii. p. 420).

It so happens that Hildebrand's statement (according to Darwin) refers to Aristolochia, not to Arum. But even if it had referred to Arum it would only have illustrated another piece of inaccuracy of the Field Naturalist. What was shown by Hildebrand (as quoted by Darwin) to be incorrect was the old statement that flies which enter the flowers *never escape*, whereas the Field Naturalist seems to consider it an argument against imprisonment occurring at all. His own observations are on a level with this loose treatment of the problem, for he does not say in which of Delpino's stages were the

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[1] Often described as ovaries.

Arums in which he failed to find imprisoned flies. He is clearly ignorant of Knuth's and Müller's positive statements as to the presence of imprisoned insects, and of Müller's description of the flies flying vainly against the imprisoning hairs. He describes the stigmas as covered with pollen after the anthers of the same spadix have burst—which is by no means surprising since the stigmas secrete nectar after they have ceased to function.

He throws in the gratuitous guess that the dead flies sometimes found at the bottom of the prison are killed by feeding "on the intensely acrid juice which, as is well known, is secreted by the tissues of the flower." As a matter of fact, precisely the opposite is known, namely, that "juice" of the plant is not acrid, the irritating effect of the tissues as a whole being due to minute pointed crystals. He concludes that Arum is "a purely self-fertilised flower." To one with any knowledge of the subject this statement, appended as a justifiable conclusion from such an array of arguments, is enough by itself to condemn the author.

Chapter xxiii., p. 190, is headed "Trimorphic Flowers. The cleistogamic flowers directly disprove the theory."[1] The hasty reader might suppose that the theory in question is the Field Naturalist's own hypothesis that nets are a cause of sterility. For if sterility can be produced by keeping the pollen from sun, rain, wind, &c., as the Field Naturalist states to be the case, then surely a cleistogamic flower, in which the andrœcium is shut up within the corolla (a covering much more impervious than a net), must be completely sterile, more especially as the reproductive parts are more or less in the dark, a condition known to produce sterility in chasmogamic flowers. This does not occur to our author, who calls the cleistogamic flower Nature's "own natural net."

The Field Naturalist completely misunderstands Darwin's point of view about cleistogamy, which, by the way, is also the view of biologists generally. Cleistogamy is an economical arrangement for securing fertilisation at any price; it is important that cross-fertilisation shall take place, but it is still more important that seedlings of any parentage should be produced. Floral structures are compromises between the two extreme forms, cleistogamy and diœciousness, in one of which offspring is assured, in the other the offspring, *if any*, is cross-fertilised. The existence of cleistogamy, instead of being fatal to "the theory," is a most instructive part of the body of facts on which the modern view is founded. Why the Field Naturalist supposes that "cleistogamic flowers directly disprove the theory," especially in the case of trimorphic plants, is not obvious, for the meaning of cleistogamy is the same in any class of flowers. We fail to see that his discussion throws any light on the subject. The only point which is worthy of notice is a quotation (p. 191) from Darwin's "Forms of Flowers," which has several copyist's mistakes, and, moreover, contains interpolated words which do not occur in the original, the whole being within inverted commas. It is this sort of treatment of Darwin's text that makes it almost impossible to read the Field Naturalist. We can never know whether the quotations are correctly given, and life is not long enough for the verification of his innumerable citations. There is, however, little in the book but quotations and criticism, and when the reader distrusts the quotations and can see no value in the criticisms, the task of getting through the book becomes unbearable.

We would urge the author to give up his barren attempt to discredit work of such perennial value as Darwin's by niggling bookish methods. Let him rather imitate Darwin's life-long habit of absolutely honest experiment, coupled with broad-minded discussions in which all facts and considerations which oppose his views are brought into full prominence. Then, and not until then, can we take his writings seriously.

CHRONOMETRY.

Exposition universelle de 1900. *Congrès international de Chronométrie. Comptes rendus des Travaux, Procès-verbaux, Rapports et Mémoires.* Pp. xl + 254. (Paris: Gauthier-Villars, 1902.)

AMONGST the numerous congresses at Paris in 1900 was one on chronometry, of which the work under review is the official publication. In addition to the "minutes" of the meetings, which include abstracts of the communications, it gives the full text of more than thirty papers and reports. These deal with such subjects as the testing of watches and chronometers, the decimalisation of time, questions of units and standards, topics of historical or current interest in horology, the description of novel instruments or materials, and mathematical and physical investigations bearing on chronometry.

M. de Vanssay, one of the secretaries to the Congress, gives an account, pp. 5–12, of the tests applied to watches and chronometers at the chief testing observatories. On pp. 153–156 is the report of a commission appointed to consider the question of watch tests, with a view to securing uniformity at different places. The commission confined its attention to the regulations in vogue at Geneva, Kew and Besançon, which are similar in general character, and to new regulations proposed for Neuchatel. While generally favourable to the Geneva-Kew-Besançon rules, the majority of the commission preferred the Neuchatel method of dealing with the results obtained at different temperatures. The commission recommends the addition of a two days' test with the watch vertical pendant *down*, excessive difference between the rate in this position and in the other vertical position pendant up to be a cause for rejection. It makes other recommendations tending to increase the severity of the tests. It recommends that the marks obtained by a watch be given only in the official list of the testing institution, and expresses a wish that all observatories should assign marks according to some common scheme.

A second subject considerably discussed was the decimalisation of time, papers on this topic occupying pp. 116–145. M. Guyou would accept the existing hour and subdivide it decimally; but he would do so only in the case of clocks or chronometers, "tropomètres," used for astronomical or nautical work, whilst the general public would be left to the existing clock or "garde-temps." M. de Rey-Pailhade is more advanced, though his argument that the metre is "admirablement proportionée à la taille de l'homme" rather savours of

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[1] We have omitted the letter "D" which forms part of the title, and shows that the chapter continues the previous section C.